Yes Fellows, Most Human Reasoning is Complex *

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Abstract. This paper answers the philosophical contentions defended in (Horsten and Welch, 2007). It contains a description of the standard format of adaptive logics, analyses the notion of dynamic proof required by those logics, discusses the means to turn such proofs into demonstrations, and argues that, notwithstanding their formal complexity, adaptive logics are important because they explicate an abundance of reasoning forms that occur frequently, both in scientific contexts and in common sense contexts.

Keywords: Adaptive logics, paraconsistent logics, dynamic proofs, decision methods.

1. Aim of This Paper

A recent issue of Synthese contains a paper by Horsten and Welch (2007) on adaptive logics. The paper comprises results on the complexity of two adaptive logics, states that Batens made two mistaken claims, and attaches some philosophical comments to the complexity results. Meanwhile, one of the complexity results was shown mistaken (Verdée, 200x), but the others may be generalized to most adaptive logics in standard format—the standard format is described in Section 2. Horsten and Welch are right on one of Batens' claims, not on the other. Their philosophical comments, however, are severely misguided. The comments illustrate a deep misunderstanding about the nature and function of logics for *defeasible* reasoning forms, in other words for most human reasoning. As the misunderstanding is by no means peculiar for Horsten and Welch, it seems worthwhile to consider the matter in a systematic way.

The central claim we want to dispute is that adaptive logics are too complex to serve as an explication for actual human reasoning. Horsten and Welch presuppose that derivability is a simple relation, much simpler than, for example, truth. According to their results, the complexity

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of adaptive consequence relations, or rather of the consequence sets, is Σ^0_3 (for some infinite propositional premise sets). This brings them to their central claim. We find this claim baffling. Apparently they live in a place that only remotely resembles planet earth, where decisions are based on provisional judgements and where the formal explication of most reasoning is complex.

We also have several minor complaints about Horsten and Welch (2007). We shall present these where they seem most appropriate. Two minor complaints are best mentioned from the outset. The first concerns the title: "The undecidability of propositional adaptive logic". It is usually said that propositional **CL** (Classical Logic) is decidable, by which one means that $A_1, \ldots, A_n \vdash_{\mathbf{CL}} B$ is decidable for all $A_1, \ldots, A_n$ and B. Propositional adaptive logics in standard format are decidable in precisely the same sense.[1] The consequence relations of these logics are *undecidable* if the premise set is infinite. But so is the consequence relation of propositional **CL**. So the title comes to blaming Kripke for being single brained.

The second minor complaint concerns the presentation of adaptive logics. Twenty five years ago, there was a single adaptive logic. Today there is a multitude of extremely diverse logics that share the same formal structure. On the road, adaptive logicians had to adjust their terminology to new insights and had to find ways to systematize the growing domain. Horsten and Welch mix terminology from the last twenty five years and intersperse it with terminology of their own. They present things in a weird and idiosyncratic way, for example defining inference rules in terms of truth tables. This forces us to describe adaptive logics from scratch according to present standards.

As the adaptive logic program is application driven, it would have been nice to start with a section describing some of the reasoning forms that are explicated by adaptive logics. This would have provided philosophical motivation. Adaptive logics are intended to describe, in a strictly formal way, reasoning forms that frequently occur both in everyday contexts and in scientific reasoning. Limitations of space forced us to postpone the examples of such reasoning forms to Section 5. In Section 2, we shall present the standard format for adaptive logics and we introduce the two specific logics that Horsten and Welch criticize. The standard format is the common structure of nearly all adaptive logics. We cannot refer to Horsten and Welch's paper for this purpose, because their description is idiosyncratic and concerns two logics only.

[1] This is not fully precise. All propositional adaptive logics studied so far are decidable in this sense, including the logics referred to by Horsten and Welch. However if the lower limit logic (see Section 2) is undecidable, then so will be the propositional adaptive logic.

The contentions of Horsten and Welch will be presented in Section 3. In Section 4, the dynamic proofs of adaptive logics will be considered and Horsten and Welch's misunderstandings in this connection will be spelled out. This section is essential for understanding the relation between reasoning (the *explicandum*) and adaptive logics (the *explicatum*). In the central Section 5, (i) we shall discuss the need for defeasible reasoning forms, their complexity, and the implications for their explication in terms of logics, and (ii) we shall show that Horsten and Welch's objections are misguided.

2. Adaptive Logics in Standard Format

Adaptive logics adapt themselves to the premise set they are applied to. *The logic* adapts itself: it depends on the premise set whether a specific application of an inference rule is or is not correct with respect to the premise set. The present most attractive description of adaptive logics is called the standard format, appearing from (Batens, 2001) on and most extensively studied in (Batens, 2007), to which we refer for details and metatheoretic proofs. Nearly all known adaptive logics have been phrased in standard format, which has major advantages as will become clear below. The two logics mentioned in (Horsten and Welch, 2007) are in standard format.

An adaptive logic **AL** is defined by a triple:

1. A *lower limit logic* **LLL**: a reflexive, transitive, monotonic, and compact logic that has a characteristic semantics and contains **CL** (Classical Logic).[2]

2. A *set of abnormalities* Ω : a set (or union of sets) of **LLL**-contingent formulas, characterized by a (possibly restricted) logical form F.

3. An *adaptive strategy*: Reliability or Minimal Abnormality.

The lower limit logic is the stable part of the adaptive logic; anything that follows from the premises by **LLL** will never be revoked. The abnormalities are formulas that are presupposed to be false, 'unless and until proven otherwise'. Strategies are ways to cope with derivable

[2] This is realized by adding classical logical symbols (those having the same meaning as in **CL**) to the language. These will be written as $\check{\sim}$, $\check{\vee}$, $\check{\exists}$, etc. The classical symbols have mainly a technical use and are not meant to occur in the premises or conclusions of standard applications.

disjunctions of abnormalities: an adaptive strategy picks one specific way to interpret the premises as normally as possible.[3]

The predicative version of the logics considered in (Horsten and Welch, 2007) is defined as follows. The lower limit logic is **CLuN** (*C*lassical *L*ogic allowing for gl*u*ts with respect to *N*egation), viz. full positive **CL** with $(A \supset {\sim}A) \supset {\sim}A$ added as the only axiom for the standard negation, and extended[4] with classical negation $\check{\sim}$—see note 2. While $A \vee {\sim}A$ is a **CLuN**-theorem, $A \wedge {\sim}A$ is **CLuN**-contingent. The set of abnormalities Ω comprises all formulas of the form $\exists(A \wedge {\sim}A)$ (the existential closure of $A \wedge {\sim}A$).[5] The strategies are respectively Reliability and Minimal Abnormality—see below. The resulting adaptive logics will be called $\mathbf{CLuN}^r$ and $\mathbf{CLuN}^m$.

Incidentally, if the lower limit logic is extended with an axiom that declares all abnormalities logically false, one obtains the *upper limit logic* **ULL**. If a premise set Γ does not require that any abnormalities are true, the **AL**-consequences of Γ are identical to its **ULL**-consequences. The upper limit logic of $\mathbf{CLuN}^r$ and of $\mathbf{CLuN}^m$ is **CL**.

In the expression $Dab(\Delta)$, Δ will always be a finite subset of Ω and $Dab(\Delta)$ will denote the *classical* disjunction (see note 2) of the members of Δ. $Dab(\Delta)$ is called a *Dab*-formula. $Dab(\Delta)$ is a *minimal Dab-consequence of* Γ iff $\Gamma \vdash_{\mathbf{LLL}} Dab(\Delta)$ whereas $\Gamma \nvdash_{\mathbf{LLL}} Dab(\Delta')$ for any $\Delta' \subset \Delta$. Where $Dab(\Delta_1)$, $Dab(\Delta_2)$, $\ldots$ are the minimal *Dab*-consequences of Γ, $U(\Gamma) = \Delta_1 \cup \Delta_2 \cup \ldots$; $U(\Gamma)$ is the set of abnormalities that are *unreliable* with respect to Γ. Where M is a **LLL**-model, $Ab(M)$ is the set of abnormalities verified by M.

DEFINITION 1. *A* **LLL**-*model* M *of* Γ *is* reliable *iff* $Ab(M) \subseteq U(\Gamma)$.

DEFINITION 2. $\Gamma \vDash_{\mathbf{AL}^r} A$ *iff* A *is verified by all reliable models of* Γ.

So a **LLL**-model of Γ is reliable iff it verifies only abnormalities that are unreliable with respect to Γ anyway. According to an adaptive logic that has Reliability as its strategy, the semantic consequences of Γ are the formulas verified by all reliable models of Γ.

DEFINITION 3. *A* **LLL**-*model* M *of* Γ *is* minimally abnormal *iff there is no* **LLL**-*model* M' *of* Γ *such that* $Ab(M') \subset Ab(M)$.

[3] Apart from Reliability and Minimal Abnormality, several strategies were developed mainly in order to characterize consequence relations from the literature in terms of an adaptive logic. All those strategies can be reduced to Reliability or Minimal Abnormality under a translation.

[4] Suitable axioms are $(A \supset \check{\sim}A) \supset \check{\sim}A$ and $A \supset (\check{\sim}A \supset B)$.

[5] So, for the propositional fragment, Ω comprises all formulas of the form $A \wedge {\sim}A$.

DEFINITION 4. $\Gamma \vDash_{\mathbf{AL}^m} A$ *iff A is verified by all minimally abnormal models of* Γ.

So a **LLL**-model M of Γ is minimally abnormal iff no other **LLL**-model of Γ verifies (set theoretically) less abnormalities than M. According to an adaptive logic that has Minimal Abnormality as its strategy, the semantic consequences of Γ are the formulas verified by all minimally abnormal models of Γ.

An annotated **AL** proof consists of lines that have four elements: a line number, a formula, a justification and a condition. Where

$$A \qquad \Delta$$

abbreviates that A occurs in the proof on the condition Δ, the (generic) inference rules are:

PREM If $A \in \Gamma$:

$$\begin{array}{ll} \dots & \dots \\ \hline A & \emptyset \end{array}$$

RU If $A_1, \dots, A_n \vdash_{\mathbf{LLL}} B$:

$$\begin{array}{ll} A_1 & \Delta_1 \\ \dots & \dots \\ A_n & \Delta_n \\ \hline B & \Delta_1 \cup \dots \cup \Delta_n \end{array}$$

RC If $A_1, \dots, A_n \vdash_{\mathbf{LLL}} B \check{\vee} Dab(\Theta)$

$$\begin{array}{ll} A_1 & \Delta_1 \\ \dots & \dots \\ A_n & \Delta_n \\ \hline B & \Delta_1 \cup \dots \cup \Delta_n \cup \Theta \end{array}$$

We shall need to consider *stages* of proofs, which are lists of lines obtained by applications of the three above rules, with the usual understanding that the justification of a line should only refer to lines preceding it in the list. The empty list will be considered as stage 0 of every proof. Where s is a stage, s' is an *extension* of s iff all lines that occur in s occur in the same order in s'. A (dynamic) proof is a chain of stages. Here comes a peculiarity required by the Minimal Abnormality strategy. Normally, the extension of a stage is obtained by appending lines. This is not required here. The added lines may be inserted, provided that the justification of every line refers only to preceding lines. A line inserted between lines 4 and 5 may, for example, be numbered 4.1.[6]

That A is derivable on the condition Δ may be interpreted as follows: it follows from the premise set that A or one of the members of Δ is true.

[6] An alternative, which we shall not consider in this paper, is to renumber all lines after the insertion and to adjust the old line numbers in the justifications.

As the members of Δ, which are abnormalities, are supposed to be false, A is considered as derived, unless and until it shows that the supposition cannot be upheld. The precise meaning of "cannot be upheld" depends on the strategy, which determines the marking definition (see below) and hence determines which lines are marked at a stage. If A is only the formula of marked lines, it is considered as not derived at that stage.

We now set out to present the marking definitions. $Dab(\Delta)$ is a *minimal Dab-formula* at stage s of an **AL**-proof iff $Dab(\Delta)$ has been derived at that stage on the condition $\emptyset$ whereas there is no $\Delta' \subset \Delta$ for which $Dab(\Delta')$ has been derived on the condition $\emptyset$.[7] A *choice set* of $\Sigma = \{\Delta_1, \Delta_2, \ldots\}$ is a set that contains an element out of each member of Σ. A *minimal choice set* of Σ is a choice set of Σ of which no proper subset is a choice set of Σ. Consider a proof from Γ at stage s and let $Dab(\Delta_1)$, ..., $Dab(\Delta_n)$ be the minimal Dab-formulas at that stage. $U_s(\Gamma) = \Delta_1 \cup \ldots \cup \Delta_n$[8] and $\Phi_s(\Gamma)$ is the set of minimal choice sets of $\{\Delta_1, \ldots, \Delta_n\}$.[9]

DEFINITION 5. *Marking for Reliability: Line l is marked at stage s iff, where Δ is its condition, $\Delta \cap U_s(\Gamma) \neq \emptyset$.*

Note that at least one line on which A is derived is unmarked iff, on the present estimation of $U(\Gamma)$ (see note 8), A is verified by all reliable models of Γ.

DEFINITION 6. *Marking for Minimal Abnormality: Line l is marked at stage s iff, where A is derived on the condition Δ on line l, (i) there is no $\varphi \in \Phi_s(\Gamma)$ such that $\varphi \cap \Delta = \emptyset$, or (ii) for some $\varphi \in \Phi_s(\Gamma)$, there is no line on which A is derived on a condition Θ for which $\varphi \cap \Theta = \emptyset$.*

This reads more easily: where A is derived on the condition Δ on line l, line l is *unmarked* at stage s iff (i) there is a $\varphi \in \Phi_s(\Gamma)$ for which $\varphi \cap \Delta = \emptyset$ and (ii) for every $\varphi \in \Phi_s(\Gamma)$, there is a line at which A is derived on a condition Θ for which $\varphi \cap \Theta = \emptyset$.

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[7] Note the similarity with the definition of a minimal *Dab*-consequence of Γ. The minimal *Dab*-formulas at a stage represent an estimation of the minimal *Dab*-consequences of Γ; the estimation depends on the insights provided by the stage of the proof.

[8] $U_s(\Gamma)$ may be seen as the estimation of $U(\Gamma)$ that is provided by stage s of the proof.

[9] Let $\Phi(\Gamma)$ be defined similarly from the minimal *Dab*-consequences of Γ. It can be shown that $\varphi \in \Phi(\Gamma)$ iff there is a minimally abnormal model M of Γ for which $\varphi = Ab(M)$. $\Phi_s(\Gamma)$ may be seen as the estimation of $\Phi(\Gamma)$ that is provided by stage s of the proof.

Note that all lines on which A is derived are marked unless, on the present estimation of $\Phi(\Gamma)$ (see note 9), A is verified by all minimally abnormal models of Γ.[10]

A formula A is *derived at stage* s of a proof from Γ iff it is the formula of a line that is unmarked at that stage. Marks may come and go as the proof proceeds. So one also wants to define a stable notion of derivability, which is called *final derivability.*

DEFINITION 7. *A is* finally derived *from Γ on line l of a stage s iff (i) A is the second element of line l, (ii) line l is not marked at stage s, and (iii) every extension of the stage in which line l is marked may be further extended in such a way that line l is unmarked.*

In Definition 7, s may be taken to be a *finite stage* for both strategies. For the Reliability strategy, the definition may moreover be taken to refer to *finite extensions* only. For Minimal Abnormality the definition should be required to refer to finite as well as to infinite extensions, as was shown in (Batens, 1999, p. 479).

The intuitive notion behind final derivability is the existence of a proof that is *stable with respect to an unmarked line l*: A is derived on line l and line l is unmarked in the proof and in all its extensions. However, for some **AL**, Γ, and A, only an infinite proof from Γ in which A is the formula of a line l is stable with respect to line l. A simple example is the $\mathbf{CLuN}^r$-proof of p from $\{p \vee q, {\sim}q, (q \wedge {\sim}q) \vee (r_i \wedge {\sim}r_i), (q \wedge {\sim}q) \supset (r_i \wedge {\sim}r_i)\}_{i \in \{0,1,\ldots\}}$. Every finite stage can be extended with a formula $(q \wedge {\sim}q) \vee (r_i \wedge {\sim}r_i)$ for an i that does not yet occur in the stage. In the extension, $q \wedge {\sim}q$ is unreliable and hence line l is marked. The proof becomes stable only after $r_i \wedge {\sim}r_i$ is derived for all $i \in \mathbb{N}$. Needless to say, the existence of an infinite proof is not established by producing the proof but by reasoning in the metalanguage. This is why, from the very first paper on, Definition 7 was introduced—see also Section 4. There is an easy demonstration (Theorem 40 of Batens (200x)) that A is finally derived at a *finite* stage of a proof from Γ according to Definition 7 iff A is derived on an unmarked line l of a (possibly infinite) proof from Γ that is stable with respect to line l.

[10] The person who devises the proof has nothing to decide or even to do in connection with marking. It is governed by a definition, not by a rule. The mistaken name "rule" occurred in older papers on adaptive logics, but was corrected at least from (Batens and Meheus, 2000) on. So it is a pity that Horsten and Welch continue the confusion. Their way of proceeding moreover brings them to proofs in which certain steps are repeated an infinite number of times, as appears from the example in Section 3 below. This is not very elegant. As the proof goes on, one changes one's mind on derivability in view of the minimal *Dab*-formulas. This is best expressed by lines being marked or unmarked at a stage. Horsten and Welch's way of proceeding moreover cannot be upheld for the Minimal Abnormality strategy.

Definition 7 has an attractive game-theoretic interpretation. The proponent first produces a stage containing a line l of which A is the formula. Next the opponent extends the proof. Finally, the proponent extends the extension. The proponent wins iff line l is unmarked at this point. The proponent has a winning strategy iff she can win whatever move the opponent makes. The existence of a winning strategy is obviously established by a metalanguage reasoning.

The standard format provides an attractive systematization of adaptive logics. Apart from $\mathbf{CLuN}^r$ and $\mathbf{CLuN}^m$, many other *corrective* adaptive logics have been studied. The upper limit logic of all of them is **CL** or an extension of **CL**, for example a modal logic. With respect to the standard symbols of the language, their lower limit logic is weaker than **CL**; it is paraconsistent, some other logical symbol is defective, several symbols are defective, or some non-logical symbols are ambiguous or vague. Many other adaptive logics are *ampliative* in that their lower limit logic is **CL** or an extension of it. These include logics for inductive generalization, for abduction, for handling background knowledge, for generating questions, etc. Other adaptive logics are corrective as well as ampliative. Adaptive logics can very easily be combined in several ways.

If an adaptive logic is in standard format, the format provides the proof theory as well as the semantic characterization of the logic, as we have seen. The standard format also provides lots of metatheoretic results, including the soundness and completeness proofs and the proofs of all the interesting properties—see (Batens, 2007). It also provides *criteria* for final derivability, viz. procedures (some pertaining to proofs, others pertaining to tableaux) that enable one to decide, for specific A and Γ, that A is finally derivable from Γ—see also Section 5.

3. Horsten and Welch's Contentions

There is no point in summarizing Horsten and Welch's idiosyncratic formulation of propositional $\mathbf{CLuN}^r$ and $\mathbf{CLuN}^m$. However, there are a few things we need to mention in order to make their definitions understandable.

First of all Horsten and Welch require that, once marked, a line remains marked forever. They allow that the formula of the marked line is derived on a new line (if this is unmarked), and require that this line is appended to (the stage of) the proof. Next, they present an alternative definition of final derivability. Disregarding some unimportant idiosyncracies,[11] *their Definition 2* comes to:

> A formula A is finally $\mathbf{CLuN}^r$-derivable from a set of premises Γ if and only if there is a proof $\mathcal{P}$ of A from Γ on a certain line l, and this proof cannot be extended to a proof $\mathcal{Q}$ in which line l is marked.

By "a proof $\mathcal{P}$ of A from Γ on a certain line l" they mean that A is the formula of line l of a certain proof.

These changes, which they say to introduce "for diagnostic purposes", lead to proofs that are not lists in the usual sense—see, for example, Boolos et al. (2002)—because their length may be $\omega + 1$, etc. Also, we do not see any diagnostic use of the changes.

In order to bring their approach to $\mathbf{CLuN}^r$ closer to the approach of adaptive logicians, they present *their Definition 5*, which roughly is our Definition 7, except that they allow the stage mentioned in that definition as well as all extensions mentioned in it to have length ω. They purport to show that the restriction to a finite stage and finite extensions is mistaken (beginning of their section 3.3). They do so by means of the following example. Let $\Gamma_3 = \{p \vee q, {\sim}q, (q \wedge {\sim}q) \vee (r_i \wedge {\sim}r_i), ((q \wedge {\sim}q) \vee (r_i \wedge {\sim}r_i)) \supset (r_i \wedge {\sim}r_i) \mid i \in \mathbb{N}\}$ and consider the following $\mathbf{CLuN}^r$-proof from Γ_3. We shall not write any marks, but explain the matter immediately after the proof.

1	$p \vee q$	Prem	$\emptyset$
2	${\sim}q$	Prem	$\emptyset$
3	p	1, 2; RC	$\{q \wedge {\sim}q\}$
4	$(q \wedge {\sim}q) \vee (r_1 \wedge {\sim}r_1)$	Prem	$\emptyset$
5	$((q \wedge {\sim}q) \vee (r_1 \wedge {\sim}r_1)) \supset (r_1 \wedge {\sim}r_1)$	Prem	$\emptyset$
6	$r_1 \wedge {\sim}r_1$	Prem	$\emptyset$
...			
k	p	1, 2; RC	$\{q \wedge {\sim}q\}$
$k+1$	$(q \wedge {\sim}q) \vee (r_i \wedge {\sim}r_i)$	Prem	$\emptyset$
$k+2$	$((q \wedge {\sim}q) \vee (r_i \wedge {\sim}r_i)) \supset (r_i \wedge {\sim}r_i)$	Prem	$\emptyset$
$k+3$	$r_i \wedge {\sim}r_i$	Prem	$\emptyset$
...			
ω	p	1, 2; RC	$\{q \wedge {\sim}q\}$

Line 3 is marked when line 4 is added, and (in Horsten and Welch's setup) the mark is not removed when line 6 is added. However, after line 6, p may be derived again on a new line. This may be done infinitely many times as lines k to $k+3$ illustrate. Line k is marked when line $k+1$

[11] They consider the set of marks, which is a set of line numbers that cause the mark, as an element of a line of a proof.

is added, but p may be derived on a new line after line $k+3$ was added. Only after all minimal *Dab*-formulas, viz. all formulas $r_i \wedge \sim r_i$ have been derived, p can be derived on a line that is and remains unmarked. Note that, in our setup, line 3 is unmarked at stage 6 of the proof and there is no need to introduce line k; line 3 will be marked at stage $k+1$ of the proof, unmarked again at stage $k+3$, and so on.

For $\mathbf{CLuN}^m$ Horsten and Welch do not go into the details of proofs and do not present a marking definition, but define final derivability with respect to formulas that can be categorically derived from the premise set.[12]

As a next step, Hosten and Welch set out to study the complexity of final derivability, (recursive) infinite propositional premise sets included. For $\mathbf{CLuN}^r$ the outcome is Σ^0_3. Π^1_1 is an upper bound for $\mathbf{CLuN}^m$, but the precise outcome turns out to be Σ^0_3.[13]

We now come to the philosophical reflections that Horsten and Welch attach to these results.

Propositional adaptive logics are decidable in the usual sense: $A_1, \ldots, A_n \vdash B$ is decidable. Horsten and Welch correctly point out that Batens made a mistake when he stated the guess that decidability survives if the propositional premise set is infinite. Batens has an excuse. All interesting applications of adaptive logics to the philosophy of science concern the predicative case. There the consequence relation is not only undecidable, there even is no positive test for it (in general) as is noted in many published papers—the technicalities are clarified in subsequent sections. So Batens did not really care for infinite propositional premise sets. Still, the mistake was careless and had to be corrected.

Horsten and Welch object to the fact that some premise sets and conclusions require infinite proofs. They quote Church who, in a reaction to Zermelo, remarks that logics are explications of the concept of proof and proofs should carry finality of conviction to anyone who admits the assumptions of the proof. This requires a finitary syntactical test for the validity of proof candidates, which is impossible in infinite cases. According to Horsten and Welch, the *transfinite* character of some adaptive proofs is to be blamed for the fact that the "final proofs [of adaptive logics] do not carry finality of conviction".

Moreover, Horsten and Welch argue that the complexity of adaptive logics is even more problematic than the transfinite character of the proofs. Because truth itself is a complex notion, derivability should

[12] As we have seen in Section 2, the only difference with $\mathbf{CLuN}^r$ is the marking definition.

[13] Verdée (200x) has shown that this is mistaken: the complexity of final $\mathbf{CLuN}^m$-derivability is Π^1_1.

be comparatively simpler. Horsten and Welch refer to the moment at which the propositional relevant logic **R** was proven undecidable. At the time, this result was seen as a major problem for relevance logic, precisely because the logic **R** was supposed to explicate a common sense notion. Adaptive logicians have made a similar claim: the adaptive proofs should explicate actual reasoning processes. Therefore the adaptive proofs should be simple. Relevant logics have the excuse of a complex implication connective. The inconsistency-adaptive logics under discussion, however, have only simple classical and paraconsistent connectives, which have a straightforward two-valued semantics.

So adaptive logics are undecidable. Horsten and Welch point out that the situation is even worse. Adaptive logic is not only undecidable but even Σ^0_3-complex. Formal learning theory has taught us that an algorithm that converges to a correct answer (yes or no) for the question whether x is an element of a set of natural numbers is only available if the set is maximally Δ^0_2-complex. Adaptive logicians always stated that adaptive logics are conceived for contexts where there is no positive test, but because adaptive logic consequence sets can apparently exceed the Δ^0_2-bounds, there cannot even be a machine that generates adaptive proofs that stabilize to the right answer (if there is any). In view of this result, Horsten and Welch attack a claim by Batens, viz. that, as a dynamic proof proceeds, insights in the premises may increase and never decrease. They argue that derivability at a stage does provably not provide a good estimate of final derivability.

4. Adaptive Proofs

Consider a simple $\mathbf{CLuN}^m$-proof. Let $\Gamma = \{{\sim}p, {\sim}q, p \vee r, p \vee q, q \vee r\}$. From here on, we obviously shall present proofs in our way.

1	${\sim}p$	Prem	$\emptyset$
2	${\sim}q$	Prem	$\emptyset$
3	$p \vee r$	Prem	$\emptyset$
4	r	1, 3; RC	$\{p \wedge {\sim}p\}$
5	$p \vee q$	Prem	$\emptyset$
6	$(p \wedge {\sim}p) \vee (q \wedge {\sim}q)$	1, 2, 5; RU	$\emptyset$
7	$q \vee r$	Prem	$\emptyset$
8	r	2, 7; RC	$\{q \wedge {\sim}q\}$

Up to stage 5 of the proof, viz. before line 6 is added, no line is marked because no *Dab*-formula has been derived. At stage 6, line 4 is marked. Indeed $\Phi_6(\Gamma) = \{\{p \wedge {\sim}p\}, \{q \wedge {\sim}q\}\}$ and r has not been derived on a condition Θ for which $\{p \wedge {\sim}p\} \cap \Theta = \emptyset$. At stage 8, all lines are unmarked again.

Does 1–8 form a demonstration that $\Gamma \vdash_{\mathbf{CLuN}^m} r$? Obviously not. It is a proof in the sense that it is written according to the rules of $\mathbf{CLuN}^m$ (in view of the specific premise set), not in the sense that it is a demonstration. One knows that r is a final $\mathbf{CLuN}^m$-consequence of Γ because one sees (and can demonstrate) that 6 is the only minimal *Dab*-consequence of Γ. This information is not displayed in the proof and cannot be displayed there. Adaptive proofs in themselves are not demonstrations of the final derivability of a formula from the premises.[14] To turn an adaptive proof into such a demonstration, one needs a *reasoning at the metalevel*.

This has nothing to do with the fact that some adaptive proofs are infinite, which Horsten and Welch blame for the fact that the "final proofs [of adaptive logics] do not carry finality of conviction". This is a misunderstanding: even finite adaptive proofs from finite premise sets do not in themselves carry finality of conviction with respect to *final* derivability—they obviously do with respect to derivability at a stage.

The depth of Horsten and Welch's misunderstanding may be illustrated as follows. Consider again the proof in Section 3. This proof, notwithstanding its length, ω or rather $\omega + 1$, does not demonstrate that p is a consequence of Γ_3 on *their* Definition 2 for final $\mathbf{CLuN}^r$-derivability. Indeed, it cannot be seen from the proof that it has no extensions in which the line labelled ω is marked. Has this anything to do with the proof being *infinite*? By no means so. Replace i by 1 in Γ_3 (and remove the now pointless condition $i \in \mathbb{N}$) and consider the subproof 1–6, or rather add a line 7 that is identical to line 3 (to make Horsten and Welch happy). On *their* Definition 2 p is finally $\mathbf{CLuN}^r$-derived in this proof. And they are quite right: the matter is even decidable. But the proof does not demonstrate that p is finally derived because the proof does not contain and cannot contain the information that p is not marked in any of its extensions. So according to their Definition 2, just as much as according to our Definition 7, a reasoning at the metalevel is required to turn a proof into a demonstration.

We shall argue that such proofs form nevertheless a useful explication of certain (frequently occurring) reasoning forms. However, let us first have a closer look at the proofs.

The usual definition identifies $\Gamma \vdash_{\mathbf{CL}} A$ with the existence of a list of formulas that is obtained by applying **CL**-rules (depending on the

[14] There is an exception. That A is derived on the condition $\emptyset$ warrants that it is derivable from the premises by the lower limit logic and hence is finally derivable from the premises by the adaptive logic. This special case is similar to the general case for **CL**, which is discussed below in the text.

specific formulation), that ends with A, and in which all formulas introduced by the premise rule belong to Γ. This definition is only adequate because **CL** is compact and monotonic. In view of this, some will argue that the proofs of **CL**, as those of every logic, are only demonstrations in view of a reasoning at the metalevel. The situation of adaptive logics is special, however. For usual logics, such as **CL**, the required metalevel reasoning concerns properties of the logic. This may be provided independently of a specific premise set or conclusion. For adaptive logics one moreover needs a reasoning about specific **LLL**-consequences of the premises.

That one needs this specific information is typical for *dynamic* proofs, that is proofs in which formerly drawn conclusions may be revoked. This dynamics occurs for non-monotonic logics, but also for some monotonic consequence relations. Consider the Weak consequence relation from (Rescher and Manor, 1970): $\Gamma \vdash_W A$ iff there is a consistent $\Gamma' \subseteq \Gamma$ for which $\Gamma' \vdash_{\mathbf{CL}} A$. Clearly $\vdash_W$ is a monotonic consequence relation: every consistent subset of Γ is a consistent subset of $\Gamma \cup \Delta$ for every Δ. But as there is no positive test (see the next paragraph) for consistency, the proofs of a logic characterizing $\vdash_W$ are necessarily dynamic: that A is derived from some members $B_1, \ldots, B_n$ of Γ provides only a reason to consider A as a Weak consequence of Γ if $\{B_1, \ldots, B_n\}$ is a consistent set—this holds even if $A \in \Gamma$. Incidentally, the Weak consequence relation is characterized by an adaptive logic—see (Batens, 2000) and (Verhoeven, 2001).

Where **L** is a logic, $\vdash_{\mathbf{L}}$ is decidable iff there is an algorithm for it: a mechanical procedure that, for any Γ and A, leads after finitely many steps to the answer YES if $\Gamma \vdash_{\mathbf{L}} A$ and to the answer NO if $\Gamma \nvdash_{\mathbf{L}} A$. $\vdash_{\mathbf{CL}}$ is not decidable, but there is a positive test for it (it is semi-decidable): there is a mechanical procedure that, for every Γ and A, leads after finitely many steps to the answer YES iff $\Gamma \vdash_{\mathbf{CL}} A$ (but may not provide an answer at any finite point if $\Gamma \nvdash_{\mathbf{CL}} A$). Adaptive logics are typically meant as explications for consequence relations for which there is no positive test, as was noted in most papers on adaptive logics published after 2000. A positive test for derivability is absent because of the condition involved in the consequence relation: A is **AL**-derivable if certain other formulas are *not* derivable by the lower limit logic **LLL**. If there is no positive test for **LLL**-non-derivability, there is no positive test for **AL**-derivability. Adaptive logics explicate this in terms of conditions and marks, but the phenomenon is typical for all forms of defeasible reasoning, for example default reasoning. Quite a few forms of defeasible reasoning have been characterized by adaptive logics in standard format, usually under a translation, and it is a long term aim of adaptive logicians to do so for all such reasoning forms.

The comment following Definition 7 states that the *extensions* mentioned in the definition may be taken to be *finite* for the Reliability strategy, but that infinite extensions have to be taken into account for the Minimal Abnormality strategy. In this sense, and only in this sense, did adaptive logicians ever introduce infinite proofs. Obviously the existence of an infinite extension in which line l is unmarked cannot be established by writing it down, but only by a reasoning at the metalevel. Actually, it would not make much of a difference that one could write it down, because the definition requires a statement on extensions of all possible extensions of the stage s. Recall that this holds even for finite adaptive proofs from finite premise sets.

The proof 1–8, displayed at the outset of this section, is stable with respect to line 4 in the sense explained in Section 2. However, according to Definition 7, r is *finally derived* at stage 4 of that proof. The only extensions of 1–4 in which line 4 is marked are those in which the present line 6 occurs. They can all be extended in such a way that the present line 8 occurs in them, resulting in line 4 being unmarked. The important lesson to be drawn is that r is finally derived in 1–4 as well as in 1–8 according to Definition 7, but that only 1–8 is *stable with respect to line* 4.

This highlights the advantage of Definition 7 over a definition of final derivability in terms of stability with respect to line l. Indeed, every formula A that is finally **AL**-derivable from Γ (for every adaptive logic **AL** in standard format) is finally derived at a finite stage of a proof from Γ,[15] whereas, for some A and Γ, no finite proof from Γ is stable with respect to a line on which A is derived. Finite proofs may be written down. Infinite proofs and infinite extensions of proofs cannot be written down, but one may come to conclusions about them by a metalevel reasoning (which can be written down).[16]

Allow us a short degression at this point. It is simple enough to restrict adaptive logics to decidable cases, like finite propositional premise sets, or premise sets and conclusions that belong to decidable fragments of the (predicative) lower limit logic. As will become clear in Section 5, to do so eliminates the most interesting applications of adaptive logics. Alternatively one could define a full-blown semantics for adaptive logics and restrict the proofs to decidable fragments. In doing so, however, the derivability relation $\Gamma \vdash_{\mathbf{AL}} A$ cannot possibly be complete with respect to the semantic consequence relation $\Gamma \vDash_{\mathbf{AL}} A$. In (Batens, 1999) the soundness and completeness of $\mathbf{CLuN}^r$ and $\mathbf{CLuN}^m$ are

[15] For a more precise statement and its proof, see Theorem 39 of Batens (200x).

[16] That the metalevel considerations refer to infinitely many extensions of a stage, cannot possibly count as an objection. The entities reasoned about are certainly simpler than models.

proved; in (Batens, 2007) the proofs are generalized to all adaptive logics in standard format.

Let us turn to Horsten and Welch's contention that Batens is mistaken in claiming that Reliability requires only a reference to finite stages and finite extensions—they repeat this extensively in Section 5. They are badly wrong. *Their* Definition 2 requires infinite proofs in order to conclude (from the proof and metalevel considerations about its extensions) to final derivability. *Batens'* Definition 7 requires only finite stages and finite extensions for Reliability. *Their* Definition 5 can be safely restricted to finite proofs and finite extensions for Reliability, in which case it is identical to Batens' Definition 7. That Batens' Definition 7 is correct as it stands follows, first, from the proofs in (Batens, 1999) as well as from the generalized proofs in (Batens, 2007). It is also proven directly in (Batens, 200x). We shall not repeat these proofs here, but let us show that Horsten and Welch's alleged counterexample is not a counterexample at all. The premise set is Γ_3 (see Section 3) and the logic is $\mathbf{CLuN}^r$. Consider lines 1–3 of the proof:

1	$p \vee q$	Prem	$\emptyset$
2	$\sim q$	Prem	$\emptyset$
3	p	1, 2; RC	$\{q \wedge \sim q\}$

Every *finite* extension E of 1–3 in which line 3 is marked contains one or finitely many premises from $\{(q \wedge \sim q) \vee (r_i \wedge \sim r_i)\}_{i \in \{0,1,\ldots\}}$. An extension of E in which 3 is unmarked is obtained by adding, for each $(q \wedge \sim q) \vee (r_i \wedge \sim r_i)$ in E,

j	$((q \wedge \sim q) \vee (r_i \wedge \sim r_i)) \supset (r_i \wedge \sim r_i)$	Prem	$\emptyset$
$j+1$	$r_i \wedge \sim r_i$	$j, \ldots$; RU	$\emptyset$

which obviously results in a finite extension of E. This is true even if, as Horsten and Welch require, p is derived on a new line in the extension of E. So, returning to our way of presenting proofs, every finite extension of 1–3 has a further finite extension in which line 3 is unmarked. This warrants, by Definition 7, that p is finally derived from Γ_3 in the proof 1–3. So, contrary to what Horsten and Welch claim, their example does not show the need to refer to infinite proofs (or infinite extensions) in the definition of final $\mathbf{CLuN}^r$-derivability. They must have been so blinded by their own definition that they could not apply Definition 7.

5. The Complexity of Reasoning

Adaptive logics are not candidates for the label "standard of deduction" (if there is such a thing). They are means to characterize, in a strictly formal way, forms of reasoning that were traditionally not recognized as formal, but frequently occur in scientific contexts as well as in everyday reasoning. This should be stressed. Those reasoning forms are being applied; adaptive logics are a means to describe them in a formally decent way. Among the criteria for judging adaptive logics, adequacy with respect to the explicandum is central. The logics cannot be blamed for the complexity of the explicandum.

Adaptive logicians have analysed many concepts themselves and have argued for this analysis, for example (Batens, 1989) and (Batens, 2002) on forms of handling inconsistency or (Batens et al., 2003) on prioritized premise sets and diagnosis. To avoid any quarrels, we shall refer to concepts introduced by people at a time they never had heard of adaptive logics.

Nicholas Rescher, partly in cooperation with Ruth Manor, developed consequence relations that handle inconsistencies in a way suitable for specific applications, including the analysis of counterfactuals—see (Rescher, 1964; Rescher, 1973; Rescher and Manor, 1970) and (Benferhat et al., 1997; Benferhat et al., 1999) for a survey and study of those consequence relations, including prioritized ones. All of them are defined in terms of **CL**-derivability from maximal consistent subsets of the premises. There is no positive test for consistency.

A recent version of the theory of the process of explanation is presented by Ilpo Halonen and Jaakko Hintikka (2005). In their Section 6, they discuss the conditions on (nonstatistical) explanations (with a number of restrictions). The conditions concern an explanandum Pb, a background theory T (in which the predicate P occurs) and an initial condition (antecedent condition) I (in which b occurs). Among the six conditions are the following:

(iii) I is not inconsistent ($\nvdash_{\mathbf{CL}} \sim I$).
(iv) The explanandum is not implied by T alone ($T \nvdash_{\mathbf{CL}} Pb$).
(vi) I is compatible with T, i.e. the initial condition does not falsify the background theory ($T \nvdash_{\mathbf{CL}} \sim I$).

There is no positive test for any of the three conditions.

In Andrzej Wiśniewski's erotetic logic, for example (1996) and (1995), erotetic evocation is defined as follows: a question Q is *evoked* by a set of declarative statements Γ iff the (prospective) presupposition[17] of Q

[17] The prospective presupposition of, for example, a whether-question is the disjunction of its direct answers. Thus the prospective presupposition of "Did Mary or John or Joan come?" is "Mary came or John came or Joan came." Our slight simplification does not harm the force of the example.

is derivable from Γ but no direct answer of Q is derivable from Γ. Note that there is no positive test for **CL**-non-derivability.[18]

This short list of predicative examples can be extended *ad nauseam*. The sources are unsuspect. There is no positive test for these concepts and their complexity is greater than that of **CL**-derivability. The reasoning leading to applications of the concepts is necessarily dynamic. The same holds for all forms of defeasible reasoning, unless it is artificially restricted to decidable or semi-decidable cases.

Apart from matters already discussed, Horsten and Welch launch a number of complaints or statements that look like complaints in their Section 5. They state that "it is not an exaggeration to say that there exist no complete proof procedures for propositional adaptive logic, at least not if "proof" is understood in the usual (finitary) sense of the word." We thought that was clear from the very first paper on adaptive logics, albeit for very different reasons than the ones adduced by Horsten and Welch.

They think that "it seems improbable that our common sense notion of propositional implication is so complicated [as relevant consequence relations and *a fortiori* as adaptive consequence relations]"—by "implication" they mean the consequence relation, not the implication symbol. We return later to the complexity argument in general, but let us point out here that adaptive logics do not explicate the common sense notion of 'propositional implication', but explicate methodological concepts and common sense concepts.

A similar confusion underlies their argument from formal learning theory. Every book or paper on formal learning theory states that there are many unsolvable problems. A problem is solvable if some method, when applied to the problem, warrants that the correct answer is obtained from a certain finite point on, even if it is unknown whether the point was reached or not. That a problem is unsolvable means that there is no such method. Now consider a *kind* of problems that comprises unsolvable problems—for example a specific kind of abduction problems or a specific kind of inductive generalization problems. When confronted with a problem of such a kind, it cannot always be determined beforehand whether the problem is solvable or not. Let the problem be to determine whether all P are Q on the basis of a strict total order over a denumerable set of instances.[19] Even if not all P are Q, no P that is not Q need occur at any finite point in the order. So whether the problem is solvable depends on the order, not on the problem type.

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[18] See (Meheus, 2001) for the adaptive logic that explicates the dynamic reasoning.

[19] A strict total order over the natural numbers, need not define a list; for example: 0 2 4 . . . 1 3 5 . . . .

Many kinds of problems comprise unsolvable items. This holds for empirical as well as for mathematical kinds of problems. Only a fool would consider this a reason for giving up on all problems of the kinds, or on all problems not demonstrated solvable. Adaptive logics enable one to formulate problems in a precise and unified way and within a specific framework. The framework is different from that of formal learning theory, it presents a different approach, and it provides one with different heuristic means. But there is more. Every adaptive logic characterizes a *kind* of problems and many such kinds comprise unsolvable problems. Incidentally, this means that the kind of problems cannot be formulated by means of, for example, **CL** because its consequence relation is not sufficiently complex. An important task is to find a method *that solves all* solvable *problems of a certain type*. That is the best a method can do and such a method "has claims to the title 'rational'" (Martin and Osherson, 1998, p. 153). This is precisely what adaptive logicians realized in terms of proof theoretic procedures (see below). So what is the point of the long paragraph that Horsten and Welch devote to formal learning theory?

They complain that Batens considers derivability at a stage as an estimate for final derivability. But there is nothing wrong with the relevant quotation from (Batens, 1995). That, as the proof proceeds, "the insights in the premises provided by the proof never decrease and may increase" is correct. The quotation concerns the **LLL**-derivability of *Dab*-formulas, not the final **AL**-derivability of a formula. The insights increase whenever a new minimal *Dab*-formula is derived (either a new one or one which makes a previously derived *Dab*-formula non-minimal). Batens never said that one can derive all minimal *Dab*-formulas at any finite point. If that could be done, there would be a positive test for $\Gamma \vdash_{\mathbf{AL}} A$. Obviously the estimate cannot be brought arbitrary close to the truth. It is just the best estimate available in view of the insights provided by the present stage of the proof.

Maybe Horsten and Welch want to say that so complex consequence relations (and concepts) should not be approached in terms of proofs, but only in terms of definitions or by semantic means. If they think so, they are wrong. If $\Gamma \vdash_{\mathbf{AL}} A$ can be decided by a reasoning about the definition or by a reasoning about models, then this reasoning can be transformed to a reasoning about proofs and *vice versa*. What is the relevance of arguments from complexity for the distinction? None obviously. The complexity is a property of the consequence relation, not of the means by which one characterizes it. Nor are proofs useless in view of the semantics: proofs offer a different perspective, which is heuristically important.

Two more points deserve attention: the complexity attainable by humans and the question how consequence relations of this complexity should be handled.

If common sense inference were simple from a formal logic point of view, one wonders why logicians have been quarrelling about it ever since the 1930s. And why is a decent theory of natural languages not since long available?

Most of human inference, both in everyday situations and in the sciences, consists of explanations, abductions, inductive generalizations, raising questions, and the like. Apparently such reasoning forms are applied, with some mistakes, by systems not more complex than human brains. This does not exclude, however, that the best normative *explications* of the reasoning forms require complex formal systems, as the above examples show.[20] A human brain does not work like anything resembling a formal system (and certainly not like a semantics).

Next, consider humans applying formal systems. Which formal systems are too complex for them? Consider **CL**. If, confronted with the simple question whether a certain formula is a **CL**-theorem, someone applies the best possible procedure, he or she may never obtain an answer (in case the answer is negative). If the answer to the question is positive, it may still, say at one operation per minute, eight hours of every working day, take 10 billion years to obtain the answer. That's too complex for us. So where is the border here? Why is **CL** simple enough while adaptive logics are too complex? What about second order logic? What about Peano Arithmetic? What about arithmetic (the standard model)? Remember that there is no positive test for "is true in the standard model". What about Analysis?

Many problems of such 'disciplines' are unsolvable while others are unsolvable by human standards. But what should we conclude from this? Do we have to stop doing mathematics because most of its theories are 'too complex'? Should we stop generating explanations, abductions, predictions, generalizations and scientific theories because there is no positive test for the underlying consequence relations? Or should we declare scientific methodology a matter of taste and luck, inapt for logical systematization? The answer to all these questions is negative. This raises a less trivial question: how should we proceed with consequence relations of such complexity?

First and foremost, we should study such consequence relations in a formally decent way. That's what adaptive logicians are trying to do. So let us return to adaptive logics. Consider an adaptive logic **AL** that

[20] Incidentally, derivability at a stage, which corresponds to common sense reasoning without metalevel considerations, is not more complex than the derivability relation of the lower limit logic.

explicates a given reasoning form, whether from scientific methodology or from an everyday situation. **AL** has a semantics and a proof theory. We may be mistaken here, but we do not believe that ordinary people or ordinary scientists reason semantically, that is about models. Try it on a bus driver and a chemist, and they will stare at you.[21] Our conjecture is that people make inferences, and intersperse them occasionally with metalevel considerations (*this* follows unless *that* would follow, but I don't believe *that* follows). Hence the attention adaptive logicians paid to dynamic proofs. As we said, we may be wrong, but we are open to learn about alternatives.

As we see it, (finite) adaptive proofs explicate quite well how people handle such consequence relations. They reason for a while, occasionally review a formerly drawn conclusion (but sometimes erroneously forget to do so), and get to a provisional conclusion. This corresponds to derivability at a stage, which may be supplemented with metalevel considerations. If these are not conclusive, there is a choice: act on present insights or continue the reasoning. The decision may largely depend on time and money (and boredom).

The partial insights offered by derivability at a stage may be very useful, even if they are not conclusive. Consider Frege's set theory. Insights in this inconsistent theory (and possibly in Cantor's inconsistent set theory) led to the contemporary theories (ZF, NF, type theories, and several others), which one hopes to be consistent. We tend to believe that those insights can be explicated by adaptive logics (a short study is forthcoming), but this should not be settled here. The insights were clearly partial (corresponding to a proof at a stage only). Indeed, the Curry paradox was only discovered after ZF, NF and other major contemporary set theories were formulated.

Do adaptive logics enable one to arrive at better justifications, viz. at final derivability? They do in some cases. First, there are the decidable cases: the propositional case (for finite premise sets) and other fragments of the predicative logics (provided the finitely many premises as well as the conclusion belong to the fragment). Even beyond those fragments establishing final derivability is possible. A proof-theoretic *procedure* was devised, first for propositional **CL** by Batens and Provijn (2001), and next for propositional $\mathbf{CLuN}^r$ by Batens (2005). Results on the predicative versions and on $\mathbf{CLuN}^m$ are forthcoming, as are the generalizations to all adaptive logics in standard format. Even for undecidable fragments, the procedure forms a criterion: for certain Γ and A, it leads after finitely many steps to a positive or negative answer

[21] Also, were the presumably consistent set theories devised by thinking about the *models* of Frege's set theory?

to the question whether $\Gamma \vdash_{\mathbf{AL}} A$.[22] In order for the procedures to lead to a positive answer, A must be derived from Γ on a line l of a proof that is stable with respect to l. So, if all such proofs are only infinite, the procedures themselves are inconclusive. In some such cases one can recur to a metalevel reasoning about the procedures.

6. In Conclusion

Horsten and Welch's (2007) contains mistakes and suffers from misunderstandings. We shall not summarize these here, but shall point to conclusions that have a more general interest.

1. Most reasoning is defeasible. Many defeasible reasoning forms were described, often in a very precise way, by philosophers of science and logicians.

2. Defeasible reasoning forms are not candidates for the standard of deduction. They have nothing to do with truth-preservation.

3. Logics characterizing defeasible reasoning forms require dynamic proofs. These proofs explicate the defeasible reasoning. Final derivability can only be established by metalevel considerations.

4. Adaptive logics explicate defeasible reasoning forms and approach them within a unified framework. They should be judged in terms of their adequacy, not in terms of the complexity of the explicated reasoning forms.

5. The complexity of these logics is high, comparable to that of mathematical theories. The complexity pertains to the logic. It affects the semantics just as much as the dynamic proof theory. Moreover, the complexity affects the metalevel reasoning, not the proofs themselves, which are simple.

6. There are proof procedures that provide criteria for final derivability. In terms of formal learning theory, they solve all solvable problems of the type explicated by the adaptive logic. In view of this, the procedures provide one with a rational means to approach the type of problems.

[22] So a proof obtained by the procedure does not require a further *specific* metalevel reasoning to establish final derivability, whence it is useful to apply the procedure even in decidable cases.

7. That a reasoning form would be too complex for humans is an obscure and confused statement. Decidable reasoning forms may be too complex in practice, while highly complex consequence relations may be approached in a way that results in a rational estimate.

The last point deserves special attention. There is no positive test for final derivability. The logician should try to delineate the decidable cases as sharply as possible and warn that, in a specific case, a final judgement about final derivability is beyond reach. If this is the case in real life applications, the logician and layman alike are thrown back to deciding in uncertainty. That's life.

Even the most classical realm is not much better off. If mathematical theories are inconsistent, the literal understanding of most mathematical work, viz. in terms of **CL**, is pointless. No absolute warrant for the consistency of even Peano arithmetic is available. So here too one has to rely on a provisional judgement and, unlike what is the case for adaptive logics, the whole theory would break down if the judgement turned out to be mistaken. And yet one should not fear provisional judgements. They led to the contemporary sciences.[23]

References

Batens, D.: 1989, 'Dynamic dialectical logics'. In: G. Priest, R. Routley, and J. Norman (eds.): *Paraconsistent Logic. Essays on the Inconsistent.* München: Philosophia Verlag, pp. 187–217.

Batens, D.: 1995, 'Blocks. The clue to dynamic aspects of logic'. *Logique et Analyse* **150–152**, 285–328. Appeared 1997.

Batens, D.: 1999, 'Inconsistency-Adaptive Logics'. In (Orłowska, 1999), pp. 445–472.

Batens, D.: 2000, 'Towards the Unification of Inconsistency Handling Mechanisms'. *Logic and Logical Philosophy* **8**, 5–31. Appeared 2002.

Batens, D.: 2001, 'A General Characterization of Adaptive Logics'. *Logique et Analyse* **173–175**, 45–68. Appeared 2003.

Batens, D.: 2002, 'In Defence of a Programme for Handling Inconsistencies'. In: J. Meheus (ed.): *Inconsistency in Science.* Dordrecht: Kluwer, pp. 129–150.

Batens, D.: 2005, 'A Procedural Criterion for Final Derivability in Inconsistency-Adaptive Logics'. *Journal of Applied Logic* **3**, 221–250.

Batens, D.: 2007, 'A Universal Logic Approach to Adaptive Logics'. *Logica Universalis* **1**, 221–242.

Batens, D.: 200x, 'Towards a Dialogic Interpretation of Dynamic Proofs'. To appear in a Festschrift for Shahid Rahman.

Batens, D. and J. Meheus: 2000, 'A Tableau Method for Inconsistency-Adaptive Logics'. In: R. Dyckhoff (ed.): *Automated Reasoning with Analytic Tableaux and Related Methods*, Vol. 1847 of *Lecture Notes in Artificial Intelligence*. Springer, pp. 127–142.

[23] Unpublished papers in the reference section are available from the internet address `http://logica.UGent.be/centrum/writings/`.

Batens, D., J. Meheus, D. Provijn, and L. Verhoeven: 2003, 'Some Adaptive Logics for Diagnosis'. *Logic and Logical Philosophy* **11/12**, 39–65.

Batens, D. and D. Provijn: 2001, 'Pushing the Search Paths in the Proofs. A Study in Proof Heuristics'. *Logique et Analyse* **173–175**, 113–134. Appeared 2003.

Benferhat, S., D. Dubois, and H. Prade: 1997, 'Some Syntactic Approaches to the Handling of Inconsistent Knowledge Bases: A Comparative Study. Part 1: The Flat Case'. *Studia Logica* **58**, 17–45.

Benferhat, S., D. Dubois, and H. Prade: 1999, 'Some Syntactic Approaches to the Handling of Inconsistent Knowledge Bases: A Comparative Study. Part 2: The Prioritized Case'. In (Orłowska, 1999), pp. 473–511.

Boolos, G. S., J. P. Burgess, and R. J. Jeffrey: 2002, *Computability and Logic*. Cambridge University Press. (Fourth edition).

Halonen, I. and J. Hintikka: 2005, 'Toward a Theory of the Process of Explanation'. *Synthese* **143**, 5–61.

Horsten, L. and P. Welch: 2007, 'The Undecidability of Propositional Adaptive Logic'. *Synthese* **158**, 41–60.

Martin, E. and D. N. Osherson: 1998, *Elements of Scientific Inquiry*. Cambridge, Mass.: MIT Press.

Meheus, J.: 2001, 'Adaptive logics for question evocation'. *Logique et Analyse* **173–175**, 135–164. Appeared 2003.

Orłowska, E. (ed.): 1999, *Logic at Work. Essays Dedicated to the Memory of Helena Rasiowa*. Heidelberg, New York: Physica Verlag (Springer).

Rescher, N.: 1964, *Hypothetical Reasoning*. Amsterdam: North-Holland.

Rescher, N.: 1973, *The Coherence Theory of Truth*. Oxford: Clarendon.

Rescher, N. and R. Manor: 1970, 'On inference from inconsistent premises'. *Theory and Decision* **1**, 179–217.

Verdée, P.: 200x, 'Adaptive Logics Using the Minimal Abnormality Strategy Are Π^1_1-complex'. Forthcoming.

Verhoeven, L.: 2001, 'All Premisses are equal, but some are more equal than others'. *Logique et Analyse* **173–174–175**, 165–188. Appeared 2003.

Wiśniewski, A.: 1995, *The Posing of Questions. Logical Foundations of Erotetic Inferences*. Dordrecht: Kluwer.

Wiśniewski, A.: 1996, 'The logic of questions as a theory of erotetic arguments'. *Synthese* **109**, 1–25.